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woven. If mathematical elaboration is to serve any useful purpose in showing how evolutionary progress is made the nature of the machine, the specific organization or speciety of the organic world, must be recognized.

O. F. COOK

WASHINGTON, D. C.,

March 15, 1912

ON FAIRNESS AND ACCURACY IN SCIENTIFIC REVIEWING

TO THE EDITOR OF THE AMERICAN NATURALIST: Any one who takes a turn at the critical hoe with the object of ridding the biological field of some of the noxious products of fertile imaginations untrammeled by quantitative facts must expect just the sort of attack which appears in your recent issue (AMER. NAT., March, 1912, p. 165).

1. Dr. Spillman has not felt the purpose, methods or results of my paper worth statement. Instead he illustrates by it the "noticeable degree of correlation between positiveness of statement and inaccuracy of statement." And for the reason: "in Dr. Harris's paper he represents me as having cited the fact [sic] that these genotype norms [sic] form a frequency curve [sic] as proof of the genotype hypothesis [sic]."

One excuses the minor inaccuracies and would be glad to pass over the whole assertion with the simple comment that it seems unjust, but to protect himself against further accusations of "inaccuracy of statement" he must add, it is not true.

What I did do was to cite Dr. Spillman among three others in substantiation of the opening sentence, "Several times recently we have been told that the means of a character in a series of pure lines form a 'Quetelet's curve.'" I based this on his statement concerning pure lines, "They not only do not differ in all their characters as the *Oenothera* mutants do, but their norms present a regular series coming under 'Quetelet's Law'" (AMER. NAT., Vol. 44, p. 760). Surely no injustice has been done so far. Later in the paper I did make a statement (which still holds true) remotely similar to the one quoted above, and said specifically "A case in point is a paper by Roemer." There was no reference whatever to Dr. Spillman—expressed, suggested, intimated, implied, . . . or intended.

2. Although it is clearly without any justification, the foregoing criticism would, it seems to me, have gained in strength by specificity and moderation of statement. But Dr. Spillman con-

tinues: "I have not been able to find time to look up other similar citations to see whether the same inaccuracy applies to them." There are *four* "similar citations": one to a paper Dr. Spillman has already read, or at least reviewed for the *AMERICAN NATURALIST* (Vol. 44, p. 761), one to a few lines in the German *Zeitschrift* for genetics, one to thirteen lines in the *AMERICAN NATURALIST* (Vol. 45, p. 423) reviewing the fourth, which has again been considered in these pages (*AMER. NAT.*, Vol. 45, pp. 686-700).

3. Dr. Spillman reiterates: "It is now fairly well established that the norms of a group of related genotypes can, in some cases at least, be arranged in a frequency curve." Thus, he tells us, genotypic differences fall under de Vries's category of fluctuating variation, while in discontinuous variation "the norms can not be thus arranged."

Personally, I have not the slightest prejudice against these conclusions, but I can not accept them without proof. Dr. Spillman cites none. *So far as I have been able to ascertain* there is not a single series of trustworthy quantitative data in support of these pregnant generalizations.

4. I acknowledge my fault in omitting *homozygous*. This was a serious blunder on my part! By including it, one can always reason in a circle and to prove his preconceptions assume that the original ancestors of a line were or were not homozygous, according to the outcome of his experiments. This is the loop-hole through which the supple genotypist can always crawl when the evidence on the other side gets a little too strong. Again, in the genotypic ritual, "I wish to publicly repent."

5. But is not the reviewer a little over-zealous when he continues, "the definition is further inaccurate in including clonal varieties under the definition of genotype"? In doing this I merely followed the example of the best specialists. My paper was correct in terminology, as I believe it still is in facts, when it went to press. Several months after the paper Dr. Spillman is criticizing appeared, changes in terminology were forced by "the dictator of the whilom orthodox" genotypic "school"!

J. ARTHUR HARRIS